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 SUBJECT Analysis of Soviet Article on Symbiosis of
 Viruses and Microbes

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comments on the Soviet article entitled, "On the Symbiosis of Viruses and
 Microbes," by L. A. Zil'ber, Uspekhi Sovremennoi Biologii, 22: 81-100, 1952.

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1. This article begins with the usual references to historical figures in Soviet science and their contemporary successors, together with the customary allusion to the dialectic method. There follow several statements totally without foundation and a number of notable non-sequiturs. For example, on page one, it is stated that the viruses in the opinion of most investigators are, "deprived of the possibility to exist in an outer natural medium." In actual fact, no one denies that viruses may exist extracellularly, they merely cannot by definition multiply in the "outer medium" which is quite a different thing. Furthermore, the fact that viruses may occasionally come in contact with microorganisms is no indication, taken alone, that "special attention must be paid to their interaction with the microbes" (page 2, paragraph 3). On cross infection, viruses also come in contact with argon in the atmosphere, but no one assumes this to have any particular significance. It appears that the author regards adsorption of viruses by bacteria as important because he believes in the Lamarckian view that the viral heredity can be modified by "contact" (page 3, paragraph 2). There are some exceptional instances of an effect on bacteriophages dependent on the genetic properties of their hosts [redacted] excluded. Presumably Dr. Zil'ber is not writing about phages, but about viruses that infect animal tissues.
2. The experiments mentioned on pages 4 and 5 discuss the adsorption of viruses by yeast cells. While it may be true that the cells were able to remove virus particles from their environment, a more convincing experiment would have shown that virus particles were concentrated on the yeast surface, rather than merely present. Furthermore, a useful control would be repetition of the experiment with inert particles in place of the yeast cells, to see if loss of virus in the supernatant was the consequence of adsorption, or of inactivation. These experiments of Turevich and Ianushevich were

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evidently not performed as carefully as those of Zeitlenk described on page 6.

3. In reference to the work of Kling and collaborators (page 9) the author appears willing to accept the possibility that poliomyelitis virus multiplies in sewage by growth within protozoa of the genus Bodo. Although science advances by an open minded attitude, the role of protozoa is here only a hypothetical device intended to explain a high titer of polio virus in the sewage. It is well known that most people infected with polio will not show clinical symptoms, and hence the assumption of a protozoan host capable of supporting viral multiplication may not be required.
4. On page 10 the author draws a distinction between physico-chemical processes as opposed to biological processes. Such a distinction is regarded as invalid by most Western scientists. Up to this point there has been no evidence to support the authors thesis, that adsorption contributes to adaptation of the virus.
5. Most of the experiments described on pages 11-16 are concerned with the attempts to cultivate the virus of variola on yeast cultures. Very few of the experiments are quantitative in nature, but depend at best on the somewhat imprecise test of activity of specific dilutions. Quantitative methods for the study of numbers of infective particles, long used by bacteriophage workers, have only recently been developed for the titration of animal viruses. The observation of Tul'shinskaja (page 14) that the virus in yeast cultures was detected up to the 44th generation, but control sowings from bouillon cultures became negative in the 3rd-4th sowing, does not prove virus propagation. It is entirely possible that the conditions when yeast is present are more favorable for the survival of virus, a possibility the author does not seem to consider important. A virus preparation of high titer could be diluted many times on transfer and still produce an infection.
6. Numerous additional work is cited to support the author's theory of symbiosis. It would be easier to evaluate these pages if fewer references were cited and more critical attention was directed to their evaluation. The author has made an effort to include some references that contradict his beliefs.
7. There are certain fairly simple conclusions that emerge from a study of "General Results" on pages 28-34 of the Zilber communication, and that are apparent to any microbiologist:

The material discussed by the author is notable for the fact that about 85% of the references appeared in the literature before 1940, notwithstanding the keen interest shown by microbiologists of all countries in the possible extracellular or non-specific cellular propagation of viruses. For example, Krueger, in this country, has recently claimed that bacteriophage titers can be raised extracellularly, but his work probably involves activation rather than duplication. There is no doubt that unsuspected symbiosis between virus and cell may well exist, since it is usual for a virus-cell relationship to be detected only if the adjustment between virus and host is comparatively poor, i.e., there is a definite pathology. On the other hand, the remarkable thing about the relationships between viruses and their hosts is the specificity involved. A human strain of virus might grow in the tissues of monkeys, but is hardly

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likely to be maintained in the genetically unrelated cells of yeast or bacteria. As previously stated, it appears far more probable that yeast cells provide an extracellular environment highly favorable for the survival of some kinds of viruses. The virtues of yeast extract as a contributor to the growth or survival of fastidious microorganisms are well known to bacteriologists.

In the last analysis, modern knowledge of viral growth maintains that the virus draws upon intracellular amino acids, purines and pyrimidines for the synthesis of its replicating units, and that the host range is so specific that even closely related species may not serve as host. The authors complete lack of knowledge of modern biochemistry and his heavy emphasis on outdated literature indicate that the paper is not worth serious attention. His idea is neither original nor esoteric, and would have been long since confirmed by many microbiologists, if capable of quantitative verification. The numerous qualitative experiments described are for the most part without means for critical evaluation. Since some Russian scientists are undoubtedly very capable, it would be unwise to take this as a typical example of Soviet science.

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